herbarium were saved, passing into the hands of Nicola Cirillo (1671–1734), a physician and botanist who possessed a private botanical garden and was a Fellow of the Royal Society of London, for which Society he collected data on the climate of Naples, and wrote a treatise on the application of cold in the treatment of fevers. Remaining in the Cirillo family, the herbarium was finally bequeathed to the celebrated botanist Domenico Cirillo, who preserved these volumes as the most precious treasure in his collections. In 1783, Martin Vahl, a friend of Linnæus, saw Imperato's herbarium in Cirillo's house, and it is said that he fell on his knees in reverence before the ancient relic. In 1799, when the royalist mob sacked Cirillo's house and Cirillo himself was hanged, all his collections were dispersed, including the herbarium of Imperato. Of the nine volumes only one was saved, and finally came into the hands of Camillo Minieri-Riccio, who in 1863 published a short account of this botanical relic (C. Minieri-Riccio: "Breve notizia dell' Erbario di Ferrante Imperato," *Rendiconti dell' Accademia Pontaniana*, xi., 1863). Minieri says that Imperato's name is written in the volume.

The collections of Minieri-Riccio were finally sold to the National Library at Naples, where the volume of Imperato's herbarium may now be seen.

The volume, of 268 pages, is bound in parchment and is labelled "Collectio Plantarum Naturalium." It contains 440 plants, glued to the paper, each with one or more names. There is an alphabetical index, probably written by Imperato himself.

The authorities in the Naples library do not seem aware of the importance of the relic they possess, for the herbarium is kept as an ordinary book and the plants are exposed to inevitable damage and decay. Several of the specimens have already been eaten up by insects. ITALO GIGLIOLI.

R. Stazione Agraria Sperimentale, Rome, January 8.

A Curious Projectile Force.

I AM able to corroborate B.A. Oxon.'s letter (p. 247). In my case, the screw stopper of the bottle (inverted) rested at an angle against some books on a table. When the pressure of the gas was sufficient to force out the stopper, the bottle sprang three or four feet into the air and fell some distance off on the floor of the room. NORMAN LOCKYER.

The Principle of Least Action. Lagrange's Equations.

WHETHER good mathematicians, when they die, go to Cambridge, I do not know. But it is well known that a large number of men go there when they are young for the purpose of being converted into senior wranglers and Smith's prizemen. Now at Cambridge, or somewhere else, there is a golden or brazen idol called the Principle of Least Action. Its exact locality is kept secret, but numerous copies have been made and distributed amongst the mathematical tutors and lecturers at Cambridge, who make the young men fall down and worship the idol.

I have nothing to say against the Principle. But I think a great deal may be said against the practice of the Principle. Truly, I have never practised it myself (except with pots and pans), but I have had many opportunities of seeing how the practice is done. It is usually employed by dynamicians to investigate the properties of mediums transmitting waves, the elastic solid for example, or generalisations or modifications of the same. It is used to find equations of motion from energetic data. I observe that this is done, not by investigating the actual motion, but by investigating departures from it. Now it is very unnatural to vary the time integral of the excess of the total kinetic over the total potential energy to obtain the equations of the real motion. Then again, it requires an integration over all space, and a transformation of the integral before what is wanted is reached. This, too, is very unnatural (though defensible if it were labour-saving), for the equation of motion at a given place in an elastic medium depends only upon its structure there, and is quite independent of the rest of the medium, which may be varied anyhow. Lastly, I observe that the process is complicated and obscure, so much so as to easily lead to error.

Why, then, is the P. of L. A. employed? Is not Newton's dynamics good enough? Or do not the Least-Actionists know that Newton's dynamics, viz. his admirable Force = Counterforce and the connected Activity Principle, can be directly applied to construct the equations of motion in such cases as above referred to, without any of the *hocus pocus* of departing from the real motion, or the time integration, or integration over all space, and with avoidance of much of the complicated work. It would seem not, for the claim is made for the P. of L. A. that it is a commanding general process, whereas the principle of energy is insufficient to determine the motion. This is wrong. But the P. of L. A. may perhaps be particularly suitable in special cases. It is against its misuse that I write.

Practical ways of working will naturally depend upon the data given. We may, for example, build up an equation of motion by hard thinking about the structure. This way is followed by Kelvin, and is good, if the data are sufficient and not too complicated. Or we may, in an elastic medium, assume a general form for the stress and investigate its special properties. Of course, the force is derivable from the stress. But the data of the Least-Actionists are expressions for the kinetic and potential energy, and the P. of L. A. is applied to them.

But the Principle of Activity, as understood by Newton, furnishes the answer on the spot. To illustrate this simply, let it be only small motions of a medium like Green's or the same generalised that are in question. Then the equation of activity is

$$\text{div. } \mathbf{q}\text{P} = \dot{\text{U}} + \dot{\text{T}}\,; \tag{1}$$

that is, the rate of increase of the stored energy is the convergence of the flux of energy, which is $-\mathbf{q}\text{P}$, if $\mathbf{q}$ is the velocity and P the stress operator, such that

$$\text{P}\mathbf{i} = \mathbf{P}_1 = \mathbf{i}\text{P}_{11} + \mathbf{j}\text{P}_{12} + \mathbf{k}\text{P}_{13} \tag{2}$$

is the stress on the $\mathbf{i}$ plane. Here $\mathbf{q}$P is the conjugate of P$\mathbf{q}$.

By carrying out the divergence operation, (1) splits into two, thus

$$\mathbf{F}\mathbf{q} = \text{T}, \qquad \mathbf{G}\mathbf{q} = \text{U}. \tag{3}$$

Here $\mathbf{F}$ is a real vector, being the force, whilst $\mathbf{G}$ is a vector force operator. Both have the same structure, viz. P∇, but in $\mathbf{F}$ the differentiators in ∇ act on P, whereas in $\mathbf{G}$ they are free and act on $\mathbf{q}$, if they act at all.

Now when U is given, U becomes known. It contains $\mathbf{q}$ as an operand. Knock it out; then $\mathbf{G}$ is known; and therefore $\mathbf{F}$; and therefore the equation of motion is known, viz.

$$\mathbf{F} = \frac{d}{dt}(m\mathbf{q}),$$

where m is the density, or the same generalised eolotropically, or in various other ways which will be readily understood by electricians who are acquainted with resistance operators.

Of course, P becomes known also. So the form of U specifies the stress, the translational force and the force operator of the potential energy. To turn $\mathbf{G}$ to $\mathbf{F}$ is the same as turning $\text{A}\frac{d}{dx}$ to $\frac{d\text{A}}{dx}$.

If, for example, the displacement is $\mathbf{D}$, the potential energy is a quadratic function of the nine differentiants $d\text{D}_1/dx$, &c., of the components. Calling these r_{11}, r_{12}, &c.;

$$\text{U} = \tfrac{1}{2}r_{11}\frac{d\text{U}}{dr_{11}} + \tfrac{1}{2}r_{12}\frac{d\text{U}}{dr_{12}} + \ \ldots, \tag{4}$$

by the homogeneous property. Therefore, since $\dot{r}_{12} = aq_1/dy = \mathbf{i}d\mathbf{q}/dy$,

$$\dot{\text{U}} = \left(\frac{d\text{U}}{dr_{11}}\mathbf{i}\frac{d}{dx} + \frac{d\text{U}}{dr_{12}}\mathbf{i}\frac{d}{dy} + \ \ldots\right)\mathbf{q} = \mathbf{G}\mathbf{q}\,; \tag{5}$$

therefore, writing P_{21} for $d\text{U}/dr_{12}$,

$$\mathbf{F} = \mathbf{i}\left(\frac{d\text{P}_{11}}{dx} + \frac{d\text{P}_{21}}{dy} + \frac{d\text{P}_{31}}{dz}\right) + \ \ldots \tag{6}$$

$$= \frac{d\mathbf{P}_1}{dx} + \frac{d\mathbf{P}_2}{dy} + \frac{d\mathbf{P}_3}{dz}. \tag{7}$$

It is clear that the differentiants in (4) (which involve the large number 45 of coefficients of elasticity in the general case of eolotropy) are the nine components of the conjugate of the stress operator. Of course, vector analysis, dealing with the natural vectors concerned, is the most suitable working agent, but the same work may be done without it by taking the terms involving q_1, q_2, q_3 separately.

Another expression for U is $\text{U} = \frac{1}{2}\mathbf{G}\mathbf{D}$, which shows how to find $\mathbf{F}$ from U directly.

Another claim made for the P. of L. A. is that it leads to Lagrange's equations of motion. That is not remarkable, seeing that both are founded upon Newtonian ideas. I suppose Lagrange's equations can be made to lead to the P. of L. A. But the practical way of proving Lagrange's form is to derive it immediately from Newton's Principle of Activity. Thus, when there are n independent coordinates x, with velocities v, the kinetic energy T is a homogeneous quadratic function of the v's, with coefficients which are functions of the x's. This makes

$$2\mathrm{T}=v_1\frac{d\mathrm{T}}{dv_1}+v_2\frac{d\mathrm{T}}{dv_2}+\ .\ .\ .\ ; \qquad (8)$$

therefore

$$2\mathrm{T}=\frac{d}{dt}\frac{d\mathrm{T}}{dv_1}v_1+\frac{d\mathrm{T}}{dv_1}\dot{v}_1+\ .\ .\ .\ . \qquad (9)$$

But also by the structure of T,

$$\mathrm{T}=\frac{d\mathrm{T}}{dx_1}v_1+\frac{d\mathrm{T}}{dv_1}\dot{v}_1+\ .\ .\ .\ . \qquad (10)$$

So, by subtraction of (10) from (9)

$$\mathrm{T}=\left(\frac{d}{dt}\frac{d\mathrm{T}}{dv_1}-\frac{d\mathrm{T}}{dx_1}\right)v_1+\ .\ .\ .\ ; \qquad (11)$$

and therefore, by Newton, the force on x_1 is the coefficient of v_1, and similarly for the rest.

Some people who had worshipped the idol did not altogether see that the above contained the really essential part of the establishment of Lagrange's form, and that the use of the activity principle to establish the equation of motion is proper, instead of *vice versâ*. To all such the advice can be given, Go back to Newton. There is nothing in the P. of L. A., or the P. of L. Curvature either, to compare with Newton for comprehensive intelligibility and straight correspondence with dynamics as seen in Nature. It must, however, be said that Newton's third law is sometimes astonishingly misconceived and misapplied, perhaps because it is badly taught.

OLIVER HEAVISIDE.

Leonids of 1902, and Quadrantids of 1903.

CLOUDS and full moonlight seem to have impeded observations of the Leonids to a considerable extent in November, 1902. The night of November 14 was fine here, but as there seemed little probability of a display on that date—as is fully confirmed by the negative results of other observers—no extended watch was maintained. The night of November 15 turned out very unfavourable. It seemed unusually bright here about 6h. 30m. on the morning of November 16. No observations were possible in the circumstances. Even if the sky had been clear, very probably nothing unusual in the way of a meteor display would have been visible, owing to the presence of the full moon, then shining with almost maximum brilliancy. M. D. Eginitis, with three assistants, observing at Athens during the night of November 15, did not see more meteors—in fact, they counted one less—than on that of November 14, 1901, on which night the American maximum took place. Both those nights were clear, but possibly the observations may not have been equally extensive. The maximum of 1902 probably took place in America, but in the absence of reports of clear observations at a few stations on the other side of the Atlantic, it is difficult to gauge with certainty the character of the display.

The Quadrantid meteors, on the other hand, were well seen here, considering the broken character of the weather. Anticipating that the display of 1903 would occur early on the night of January 3—the maximum had been determined as due at 8h. 55m.—a watch was begun at 8h. 45m., and during the next hour or so some very fine meteors were observed. The following are the times of their appearance, and their approximate flights:—

	d.	h. m.	
Jan.	3	8 53,	from 2° west of Gemini to Orion, = 1st magnitude.
,,	3	8 56,	,, 1° east of the "Guards" to Pole Star, = 1–2 magnitude.
,,	3	9 20,	,, between Castor and Pollux to Orion, = 1st magnitude.
,,	3	9 47,	,, between the "Guards" half-way to Pole Star, = 2nd magnitude.
,,	3	9 59½,	,, 20° west of "Guards" to 10° higher up, = rich streak.
,,	3	10 0,	,, 20° west of "Guards" to Cassiopeia, = Capella.

Shortly after 10 o'clock, clouds came up from the horizon and by 10h. 15m. the whole north-eastern sky up to Gemini was covered. At 10h. 35m., that part of the sky had again cleared, and, between 10h. 40m. and 10h. 55m., eight meteors, varying from about 1st to 2nd magnitudes, were observed. They were all long-pathed, but generally not so much so as the early part of the display, nor did they seem to move in beaten tracks, as it were, like the first meteors. The direction of their flight resembled, on the whole, that of the former, but one of them (= Sirius) shot downwards for about 30° in a direction parallel to the tail stars of Ursa Major. It started from a point about 20° east of that constellation. The latter part of the display between 10h. 40m. and 10h. 55m. was the richest I have ever observed. I observed no meteors, except one or two between 9 and 10 o'clock, that could not be traced. They began to come so rapidly at 10h. 40m. that when making a note of the course of one, another would put in an appearance, and so prevent the completion of the first observation, their paths not being near any well-known stars. An interval of quiescence for a few minutes would then follow, when the phenomenon would be again repeated as before. At 11 o'clock, the sky became again clouded and a heavy shower of rain terminated open-air observation. Between 12h. and 12h. 20m., two more were seen through a window, of about the 3rd magnitude, one on either side of the tail stars of Ursa Major; then clouds once more intervened.

JOHN R. HENRY.

Dublin.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE fifty-second annual meeting of the American Association was held at Washington, December 29 to January 3, and was in many respects the most successful meeting ever held in the fifty odd years of the existence of the Association. As pointed out in the article in NATURE of July 24, 1902, in the account of the Pittsburg meeting of last June, this is practically the first time in which the Association has met during the winter since the close of the Civil War, and in this meeting culminated the prolonged efforts of a special committee of the Association, of which Dr. Charles Sedgwick Minot was chairman, to bring about an agreement among the scientific and other learned societies and the leading universities and other institutions of learning in the United States to set apart the week in which the first of January falls as a "Convocation Week," and in this week to bring together at one place as many as possible of the scientific societies. This culmination of the efforts of Dr. Minot's committee was eminently satisfactory. The meeting was a great success, and the institution of Convocation Week has apparently been established under the most favourable auspices.

Dr. Ira Remsen, president of Johns Hopkins University, presided over the Washington meeting, and the retiring president, the noted astronomer, Prof. Asaph Hall, U.S.N., delivered his address on the opening night of the session. His subject was "The Science of Astronomy," and it was published in full in our last week's issue.

The local arrangements for the meeting were complete, and the President of the United States acted as honorary president of the local committee, the active chairman being Dr. C. D. Walcott, Director U.S. Geological Survey, and the local secretary Dr. Marcus Benjamin, U.S. National Museum.

The addresses of the vice-presidents of the different sections were given in the afternoon of Monday, December 29, as follows:—

Prof. G. W. Hough before the Section of Mathematics and Astronomy, on "The Physical Constitution of the Planet Jupiter." Prof. Franklin before the Section of Physics, on "Limitations of Quantitative Physics." Prof. Weber before the Section of Chemistry, on "Incomplete Observations." Prof. Culin before the